

XXVI.—*A Reply to two Statements published by the Palæontographical Society, in their volume for 1852; one appearing to accuse the University of Cambridge of illiberality in the administration of its Museum; the other reflecting on the character of Professor M'Coy.* By the Rev. Professor SEDGWICK, M.A., F.R.S. &c.

*To the Editors of the Annals of Natural History.*

GENTLEMEN,

I VENTURE to request the publication of this *Reply* in the next number of your *Journal*, which is not only an excellent vehicle of scientific information, but also one of the guardians of the honour of scientific men. You can have no interest in the following statements, except so far as they have a bearing on the cause of truth.

Some time after I had seen the 'Third Part of the British Fossil Corals,' published by the Palæontographical Society in 1852, I wrote to their Honorary Secretary, and collected from his reply, that any communication from myself, in opposition to two statements made by Professor Milne-Edwards and M. Jules Haime (in the *Memoir*, just mentioned, p. 151), would probably be rejected by the Society, or, at least, published in their next volume, in a form which would not be satisfactory either to Professor M'Coy or myself. I therefore resolved to postpone my *Reply* till it might appear in the "Third Fasciculus" of the Cambridge Palæozoic Fossils, which would be published (as I then hoped) in the spring of 1853.

Meanwhile, during my engagements away from Cambridge, I had a letter from Professor M'Coy, agreeing in substance, and almost word for word, with the one which forms the most important part of this communication. Greatly do I blame myself for not having immediately sent his letter to the press. But I was anxious, at the time, to add some words of vindication for the University of Cambridge; and having no access to the Palæontographical volumes, or any other books of needful reference, I was compelled to postpone my *Reply*; and I thereby failed, unconsciously, in my duty to my friend: for I now know that he did not immediately publish his own vindication, because he thought that he had entrusted it to myself.

The plates and letter-press of our Third Fasciculus were in progress immediately after the publication of the Second (July 1852); and Professor M'Coy, who is compelled by his duties at Belfast, to leave Cambridge in the autumn, hoped to complete

the plates, by the help of an Irish artist, during the following winter. In this hope he was entirely disappointed; and on his return to Cambridge near the end of April 1853, his work was for some weeks retarded by the unavoidable absence of Mr. West, our artist. Much of the letter-press was however struck off; and on the return of Mr. West the work was pressed forward by almost incessant labour. A very vexatious, and to me a rather costly accident, so much retarded the final completion of the plates, that Professor M'Coy was again compelled to leave Cambridge; and, to our joint mortification, the publication of the Third Fasciculus was necessarily postponed till the spring of 1854.

These dates can have but little interest to the reader; but they form a part of my vindication and apology.

In like manner the First Fasciculus was more than two years in hand before its publication. Its plates of corals were, I believe, all struck off in 1849; but the work was published in May 1851: and out of this latter date an utterly groundless imputation of unfair dealing has been insinuated against Professor M'Coy.

When he had left Cambridge at the end of the summer vacation of 1853, there was no longer the shadow of a plea for any further delay in the publication of this *Reply*; and it would have been sent to the press in November or December last but for a serious attack of illness, which has made me, for almost four months, incapable of undertaking even the humblest intellectual task. Such is my apology to Professor M'Coy, and to those who are interested in the fossil history of our Palæozoic deposits; and I conclude by expressing a hope that the following statements may be thought worthy of a place in your scientific Journal.

I have the honour to be,  
Gentlemen,  
Your faithful Servant,

A. SEDGWICK.

Cambridge, March 4, 1854.

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*Reply, &c.*

The first statement on which I am called upon to bestow some words of comment I shall immediately subjoin. The second statement appears word for word in Professor M'Coy's letter: and I may here remark, that although the subjoined quotation is word for word, the *italics* are my own—the passages having

been underlined in my private copy of the volume from which it is taken.

“ Most of the *carboniferous fossils* that we have represented in the plates joined to this monograph belong to the collections of the Geological Society of London, the Museum of Practical Geology, under the direction of Sir Henry de la Beche, the Museum of Bristol, and the rich cabinet of our esteemed friend J. S. Bowerbank, Esq. We much regret *not having been able to obtain the same liberal aid* from the Museum of the University of Cambridge, and to have been therefore obliged to omit representing in this work a certain number of species, that we have not seen in any of the numerous collections so generously placed at our disposal by the great majority of the English geologists. But the omission that we here allude to is *now* of less importance than *it appeared to us, when our application to the Cambridge Museum was rejected*, for, since that time, a young palæontologist belonging to that scientific establishment, Professor M'Coy, has published very good figures of *almost all the corals that we were desirous of obtaining* communication of from the above-mentioned museum. His recent work will enable us, at least, to complete our Catalogue of the Corals found in the Carboniferous formation of Great Britain; and having gone to Cambridge *in order to see the fossils described by that gentleman*, we have easily recognized those species which we had already met with elsewhere, and can without hesitation refer most of the others to generical divisions here adopted.” (British Fossil Corals, Part 3. pp. 150, 151, 1852.)

To this passage another is affixed, in the form of a note, which is copied (as above stated) in Professor M'Coy's letter. The two passages cannot be considered apart: and what are the conclusions which any reader of common sense would naturally draw from them? That MM. Edwards and Haime had personally made an application for the loan of certain Palæozoic fossils in the Cambridge Museum which had been rejected; that this rejection was in disadvantageous contrast with the liberal conduct of all other public bodies to which they had applied; that their loss, on account of this rejection, was the less, because Professor M'Coy had (since) published and figured (First Cambridge Fasciculus, May 1851) nearly all the species of which they were anxious to have the loan; that the Professor had made an unfair use of their First Part of British Fossil Corals (*i. e.* Tertiary and Cretaceous Corals, &c. published in 1850); and that to cover his plagiarism he had misdated a portion of his own labours, and virtually stated what was not true. I think that any attentive reader must inevitably have drawn all these inferences from the passage above quoted and the note affixed to it.

Professor M'Coy may safely be left to fight his own battles; for I know that he has truth and reason on his side; and so far

as regards the University my direct reply shall not be long. I affirm, that *no application*, direct or indirect, was ever made to me, either by MM. Edwards and Haime, or by any member of the Palæontographical Society, for a loan of any part of the Cambridge Palæozoic fossils; and I was astonished when I first read the above quotation, and the note affixed to it. For till that time I had not so much as heard that the two distinguished authors had undertaken the description of the older British fossil corals, and commenced their task.

This I stated in a letter to Professor Milne-Edwards, who justified what he had written by an appeal to Mr. Bowerbank, the Honorary Secretary of the Palæontographical Society.

With that gentleman (of whom I shall ever speak with kindness and respect) I have, consequently, had a short correspondence, in which he states that Professor Edwards did come to Cambridge and applied for a loan of certain fossil corals. On this point there is no dispute or doubt. But he further states, that *after* the Professor's return from Cambridge, he (Mr. B.) endeavoured to enforce the application by a letter to myself; "*that he never wrote to me* (in behalf of the Palæontographical Society) *but once, and that once was regarding the Palæozoic fossils.*" He further states, "that shortly after having written," he met me at Ipswich, and in a *short* conversation, as we were on the point of starting to a public meeting, he again made his request for the loan of the corals\*.

Of the conversation I have not the shadow of a remembrance; but I can prove to demonstration, that his letter (above-mentioned) had reference only to the Oolitic corals. From the very first he appears to have laboured under a positive mistake as to the nature and extent of Professor Edwards's application to our museum; and one mistake inevitably led to another.

Independently of all direct evidence, what are the obvious probabilities of the case? When MM. Edwards and Haime were at Cambridge (in 1849?) they asked for the loan of certain Oolitic species, and *for no others* (Prof. M'Coy's letter, *infra*) †. Therefore any subsequent letter urging their request

\* I have no present means of fixing the date of the conversation alluded to in the text; but it must have taken place (as I collect from Mr. B.) at one of the annual Ipswich meetings which preceded the meeting of the British Association in 1851; and therefore probably in 1850.

† I cannot exactly fix the date of the visit of MM. Edwards and Haime, but Professor M'Coy informs me that it took place a considerable time before the publication of the First Part of their British Fossil Corals;—it must therefore have been in 1849 or early in 1850. He adds, in the note I have just received from him, "They made no application for Palæozoic fossils, which they knew I was publishing, and which they told me they had then no intention of touching."

must *inevitably* have been understood by myself as relating *only to certain Oolitic species*, unless the contrary were expressed, which assuredly it was not. Again, Mr. Bowerbank states that my verbal reply (at Ipswich) was considered by him "as equivalent to a refusal;" and that the subject could not afterwards be introduced to me, "without the appearance of undue impurity." All this is perfectly consistent. If the Palæontographical Society thought that I had refused the loan of the Oolitic fossils in 1849 or 1850, it was perfectly natural for them to abstain in 1851 from any request respecting the Cambridge Palæozoic fossils. In one word, I knew exactly what had taken place at Cambridge during Professor Edwards's visit, and Mr. Bowerbank did not; and hence originated that verbal misunderstanding I have just pointed out.

Since the above correspondence closed, I have had a second communication from Professor Edwards (Paris, Feb. 4, 1854), in which he now gives me the positive grounds of his statement respecting Cambridge. Among other matters he quotes a letter of Professor M'Coy (dated March 15, 1850, and now in Professor Edwards's possession at Paris) "*relative to the Oolitic corals*," and without a word of reference to any others. Professor M'Coy's letter contains a copy of a note he had just received from myself, which was painfully written with the left hand; a fact which fixes its date to March 1850, were there no other evidence. My note concludes with the following words: "Pray explain to Mr. Bowerbank that the fossils (*i. e.* 'the Oolitic corals' as they are expressly called by Professor M'Coy) are the property of the University, and are in daily use among the students, in the way of consultation, and that it is impossible for me to send them away to Paris; but all other help in my power shall be given, and every facility for making drawings, &c."

When I lately saw my left-handed note, I was afraid that it might have been written in a petulant spirit; for I was at the time in much suffering, and neither Professor M'Coy nor myself had been quite pleased with the reserve of MM. Edwards and Haime. When at Cambridge they accepted his services for a specific object, and they did not afterwards condescend to inform him or me that those services would not be wanted. There is, however, as I rejoice to find, no petulance in my note.

What took place at Cambridge (in 1849?), when it was visited by Professor Edwards and his distinguished fellow-labourer, is stated in Professor M'Coy's letter. It was my great misfortune not to be present, and I only know the facts at second-hand. But, soon afterwards, I met Professor Edwards in London, who spoke in courteous and, as I thought, in warm terms of his reception by Professor M'Coy; and I declare, with the sincerity

of perfect truth, that when we parted I had not the most distant thought that he (M. Edwards) was dissatisfied with the arrangements he had made at Cambridge, or wished to have them changed.

Had MM. Edwards and Haime thought good, in their great essay on the British Oolitic Corals (1851), to charge the University with unwonted illiberality for having "rejected" their application for certain species in our Museum, they might have done so with, at least, verbal truth; although such a charge would, I think, have been uncourteous and unjust. But having let this occasion slip, and as if to make amends for this forbearance, they published their charge in 1852 (transferring it from one memoir in which it might have appeared, with a show of reason, to another in which it ought never to have appeared), so as to make it almost incompatible with plain historical truth; and, I believe, in such a form as to have misled the Council of the Palæontographical Society. Be this as it may, a plain statement of facts would, in 1851, have had no sting, and would never have provoked a reply. But why is the charge against Cambridge taken out of its true historical place, and brought forward in another? For no purpose, which I can comprehend, except that of affording a vehicle for a very unjust insinuation against the character of one of the most honourable and devoted of the sons of natural science. Such insinuations ought never, under any circumstances, to disfigure the Transactions of a public Society. It is most true that public Transactions are not to be the vehicles of short-lived controversy, and the Council (as I now think) did right in rejecting my application to them. Their proper office is to be the great recipients of the stream of truth,—pure, sincere, and strained from every particle of malignity. In one single unhappy page they have overstepped the duties of their high and honourable office.

The *Cambridge Geological Museum* is the property of the University; and there is not a specimen in it which I call my own. Though I have collected largely during thirty-five years, and at the cost of thousands, I have collected for the public; and the public has a true interest in the administration of the Museum\*. What, then, is the nature of its administration? It is under a board of Auditors, who are governed by laws given in the founder's will. The Professor does not receive the keys of the Museum till he has signed a very *heavy bond*, which he would forfeit to the University on any culpable neglect of duty. Two

\* For a more full account of the contents of the Museum, and of the gradual formation of its very extensive collections, both British and Foreign, the reader is referred to the "blue book" of the Royal University Commission, published by authority in 1852.

Inspectors are annually appointed, who can demand the keys whenever they think right; and they do demand them yearly, so as to see that no specimens, in the arranged catalogues, are lost or out of place. No salary is ever paid to the Professor till after the report of the Inspectors has been given in to the board of Auditors. These stringent regulations have been the means of preserving the *original collection* in its integrity: and though now seldom consulted it possesses many very valuable specimens, and has a great historical and antiquarian, as well as a scientific, interest; as every one knows who has studied the old printed Woodwardian catalogues. The same regulations apply to the additional collections (made during the last thirty-five years) after they have been arranged and placed in the cabinets of the Museum. They are then liable to *Inspection*, and to all its consequences in the annual report.

During the removal (about twelve years since) of the old, as well as the modern, collection into the new museum, it was exposed for several weeks to a great risk of loss—one or two valuable specimens disappeared; but *not*, I believe, *one* from the original cabinets. Since then the modern additions have so far advanced in arrangement, and been so much increased by the bounty of academical friends, by purchase, and by my own labours, that we have now a collection of very great value, and illustrative of nearly all departments of Palæontology.

What, then, is the spirit in which this Museum has been governed? With the exception of very short intervals (necessary for cleaning and repairs) it is open six days a week during the whole year—at the rate of six hours a day during the winter, and eight hours a day during the summer, months. Academical men, and strangers, and foreigners are admitted alike without restriction—may remain during all reasonable hours—may sketch the specimens—and may turn to their own profit and instruction the vast scientific labour that has been bestowed on the collection. Artists have, more than once, been sent down to copy specimens and portions of the great series, and their applications have never been in vain. Every possible facility has been given to their labours. In all these respects the administration of our Museum dares to challenge comparison with that of any other public museum in Europe.

While any part of our vast collection has remained unarranged, I have treated it as my own property; finally, however, to be transferred to the arranged cabinets of the University; after which it becomes subject to the more stringent laws of our Museum. After their arrangement in the University cabinets, I have no right to send any specimens away from the Museum. By such an act I might forfeit a very heavy bond. Not that

I have any fears of this kind; but no man of honourable feeling wishes to owe his safety to the *forbearance* of his friends.

Spite of the regulations here alluded to, I have several times (as Professor Owen, Mr. Searles Wood, Mr. Sowerby, and Mr. Davidson and others could witness) conveyed, for a few days, very rare specimens away from our Museum to serve some scientific purpose. I did so in each instance *meo periculo*, and without consulting the authorities, as I thought the occasions might perhaps justify the irregularity and the risk. My honoured friend, the late Professor De Blainville, strongly urged me to send him, for examination, a very valuable specimen from the old Woodwardian cabinets, which had once formed a part of the ancient collection of Agostino Scilla. I could not comply with his request; but I ventured to place the specimen in the custody of Professor Owen, that good drawings and casts might be made of it for M. de Blainville's use.

After the great labour, continued for more than thirty years, in the formation of our collection, the cost bestowed on its arrangement, the perfect liberality of its administration, and the noble descriptive and scientific catalogue of our whole Palæozoic series by M'Coy, I little expected to hear a whisper of censure against us on the score of our Museum; nor should I have cared one straw for any implied censure in the passage on which I have been led to comment, had it not been followed by a very unjust insinuation against my friend; and I now request the reader's attention to his letter of explanation and defence.

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*Professor M'Coy's Letter.*

Belfast, 26th December, 1853.

MY DEAR SIR,

IN reply to your letter, relative to the observations published by MM. Milne-Edwards and Haime in p. 151 of the Third part of their Memoir on Fossil Corals, for the Palæontographical Society, reflecting unjustly on the liberality of the Cambridge Museum, and on myself, I beg to state that I published preliminary descriptions of the new Carboniferous and Oolitic corals in the collection, in the Annals of Natural History for 1849, and that some considerable time after their publication MM. Edwards and Haime came to Cambridge to see them. I was fortunately there, and spent several hours in demonstrating all my species to them; I also showed them the drawings on stone making for our plates. They were highly complimentary on all the work that had been done, and stated that they were about preparing a Monograph on Tertiary, and subsequently one on



Oolitic Corals for the Palæontographical Society, but had no immediate intention of touching the Palæozoic Corals. They then said they were desirous of figuring my newly-published Oolitic species, and asked whether the specimens would be sent to them on application. I said there was great difficulty about sending specimens out of the collection, but that if they wanted figures our artist should draw them in after-time if they liked, and that I would myself superintend them carefully. They were delighted with the suggestion, thanked me, pointed out all the specimens I was to get drawn, and the number and size of the figures, saying they would arrange, on their return, for Mr. Bowerbank to pay the artist. It must be want of memory, therefore, that betrayed MM. Milne-Edwards and Haime into the statement at the top of their page 150: "We much regret not having been able to obtain the same liberal aid from the University of Cambridge." Their lines farther on (top of page 151) are also liable to misconception, at least; as ordinary readers think the paragraph an insinuation that I knew nothing of these new corals till MM. Milne-Edwards and Haime asked for them, and that I hastened with them into print on "their application being rejected:"—the plain facts being, that these gentlemen were attracted to Cambridge by my previously published descriptions of those very corals; that they had then made no application at all; that when they came they saw our artist finishing our plates; that my part of the work was finished; that I gave them every information in my power\*, though the Cambridge work was not regularly published till after their departure.

At the foot of the same page (151) MM. Milne-Edwards and Haime mutilate a note of mine (at page 17 of the Cambridge Pal. Foss.), and thus draw so false a conclusion that they seem to have penned a wilful calumny. My note referred to is—"As these pages were passing through the press I received MM. Milne-Edwards and Haime's great English Memoir on Corals, but at too late a period to profit *materially* from the new portions not previously published in the *Comptes Rendus*." And yet MM. Milne-Edwards and Haime act as if the underlined, important, portion had not been written. Their note is as follows:—"This work (Cambridge Pal. Foss.) was published in May 1851, some months after the first part of our *Monographie des Polyptiers des Terrains Palæozoïques*, and at least a year after the distribution of the first part of our 'Description of the British Fossil Corals' to all the members of the Palæontographical Society. In the beginning of his book (p. 17) Professor M'Coy

\* Some of which (*e. g.* the existence of radiating lamellæ in Michelin's *Dendropora*) they published soon after in the *Comptes Rendus* without acknowledgement.

expresses his regret at not having become acquainted with the latter publication early enough to be able to refer to it; and we feel much gratified in seeing that the results, which Professor M'Coy appears therefore to have obtained solely from his own observations, are often so very similar to those published by ourselves a year before; even by a singular coincidence he often makes use of the same names for the divisions previously established in the first part of this Monograph."

With regard to the first statement here made, I got the Cambridge University bookseller to write to the French publisher for the exact date at which he delivered the parts of the 'Archives' containing the French memoir in question (which bears no date, and had been quoted in print by MM. Milne-Edwards and Haime long before it was published), and I received the authentic letter in reply, fixing the real date of publication as the 26th of June 1851; I did not of course receive it till July, *our work being out the May previous*. In their second statement, that our Cambridge Fasciculus of May 1851 was "published at least a year after" the First Part of their Palæontographical Memoir, they also err in a matter of fact known to every Local Secretary of the Society throughout the country; and if they mean to deny that I only received it as the sheets on which I wrote the note were passing through the press, they again not only err in a matter of fact, but grievously err in a matter of courtesy. Here I may add, that I received the volume from the Cambridge Local Secretary on the day it was sent down by the Society to him for the members. As for the concluding part of their note, in which they try to make it appear that I used their writings while stating that I did not know them,—I have already pointed out that, to serve this purpose, they have suppressed the half of my note which stated that *I had* profited by their previously published French writings; and those who examine the Cambridge work will find that in it I have repeatedly referred to MM. Milne-Edwards and Haime, and their French papers on Corals in the *Annales des Sciences* and *Comptes Rendus*, of which their English work is little more than a partial translation, and that there is no ground for their insinuation, that either by a "singular coincidence" or otherwise, their names or observations were passed for my own.

In M. Milne-Edwards's letter to you he complains only of the author's name not being put to the Orders, Classes, or Tribes, so that his cannot be distinguished from mine or any other writer's. I need only refer the members of the Palæontographical Society to their last volume (1852), where they will find the same thing done by Prof. Forbes in his Monograph; also by King (who gives his reasons) in a former volume, and by several others; and without wasting time with the reasons, I may say

that (except where mistakes might arise) I do not put an author's name to any group larger or less definite than a *genus*, and never mean to do so.

Having now refuted these aspersions at your request, which I should scarcely have done for myself, I may add, that MM. Edwards and Haime have figured and described, as new, in their 'Monographie,' several corals previously published by myself in the 'Annals of Natural History,' and that the first idle time I have, I shall write a paper on this and other *scientific* unfairnesses in their works, with which at present we have nothing to do.

I have the honour to remain,

My dear Sir,

Very truly yours,

FREDERICK M'COY.

Rev. Prof. Sedgwick.

So far as Professor M'Coy's letter bears upon matters of fact, I can give my unqualified testimony in its confirmation. There is not a more single-minded, honourable, and truth-loving man in the list of those whom I rejoice to call my friends. No other English writer has more fully and fairly quoted the works of those who have preceded him in his own line of study; and no other English writer has shown the same accurate and extensive knowledge of what may be called the literature of palæontology:—I am not so rash as to offer any estimate of the comparative merits of his classification of Fossil Corals and of that given by MM. Edwards and Haime. In one respect, however, he has an apparent advantage over them, inasmuch as his labours are more directly connected with the works of the best authors who have preceded him in investigations similar to his own.

In clearness of description the work on the Cambridge Palæozoic Corals is almost unrivalled; and the lithographic illustrations, if inferior to those executed at Paris in artistical touch, are by no means inferior to them in accuracy of details, and in the graphic delineation of those characters which give a true scientific meaning to the specimens.

The work, when finished, will contain a careful description of every English palæozoic species in the Cambridge Museum, collected during the last thirty-two years by myself and by my friends from all the old fossiliferous strata of England. To affirm that, in a work of such great extent and difficulty, the author has fallen into no mistake or error of judgement, would be an idle boast. But I do very confidently affirm, that Professor M'Coy began his task at Cambridge after a thorough scientific training of many previous years;—that with acute senses sharpened by long experience—that with a philosophical perception

of the highest requirements of natural history, and with a patience in the endurance of continued labour which has seldom had its match, he has produced a work which entitles him to the gratitude of the University, and (I dare to add) of the scientific world.

Praises thus unqualified (called forth by the circumstances which have led me to take up the pen) might seem partial or exaggerated. But I know them to be well deserved; and to confirm my own words, and to prevent any misconstruction of them, I will quote the remarks upon M'Coy's work by Professor Bronn of Heidelberg—a great palæontologist (as I surely need not tell the reader), and, at the same time, a very just but severe critic, who is not inconsiderate or prodigal in his words of praise :

“Dieses Werk ist ausserordentlich reich an scharfen Beobachtungen, fleissigen Beschreibungen und von M'Coy aufgestellten Sippen und Arten. . . . . Mit der ausländischen und insbesondere deutschen Literatur ist der Verfasser wohl bekannt, und er hat sie reichlich benützt; das Ganze ist eine der wichtigsten Erscheinungen in der paläontologischen Literatur und fortan unentbehrlich bei allen paläozoischen Studien.” . . . . “This work is extraordinarily rich in acute observations, careful descriptions, and in genera and species established by M'Coy. . . . . The author is well acquainted with the foreign, and especially with the German literature, and has made an abundant use of it; the whole is one of the most important appearances in the literature of Palæontology, and henceforward indispensable in all Palæozoic studies.” (‘Neues Jahrbuch’ by Professors Leonhard and Bronn of Heidelberg, 1853, pp. 97, 98.)

During the early progress of M'Coy's work (though repeatedly urged to do so) I studiously abstained from giving him any scheme of tabular arrangement derived from the physical groups of the Cambrian and Silurian series. I simply gave him the general facts of superposition. He, therefore, began by arranging all the groups of fossils, below the old red sandstone, as parts of one system; and for two successive years, without a single word of interruption from myself, he described them, in the printed labels and catalogues, as Upper and Lower Silurian.

In the further progress of his work he found a great palæontological break in the series, which led him to separate it into two Systems; and then, for the first time, he adopted my name Cambrian for the lower of the two. Still there was an unexplained difficulty: for in one remarkable group (called Middle Silurian in the Government Survey, and containing the greater number of the Lower Silurian rocks of Sir R. I. Murchison) were subordinate groups of strata, some of which conformed to the Silurian, and others to the Cambrian type. My own collection did not seem to sanction the establishment of the so-called

Middle Silurian group; and on the fossil evidence in the Cambridge Museum Professor M'Coy ventured to affirm his conviction, that some great sectional or palæontological error had been committed in the establishment of the middle group.

We put this conclusion to the test in 1852; and on good sectional and fossil evidence, we were enabled to break up this group into two parts, which were not only palæontologically distinct, but generally unconformable one to the other. Thus the May Hill Sandstone became at length the true base (physically and palæontologically) of the whole Silurian series; and by the interpolation of that sandstone there will be no longer any real difficulty in the tabular view which will precede the Third Fasciculus\*.

I might here (as not by any means unconnected with the subject of this communication) also dwell upon the great value of Professor M'Coy's determination of the Devonian corals, as distinguished from the Cambrian, the Silurian, and the Carboniferous: but I must forbear; and I hope to take up some of the subjects, here pointed at, in a future communication.

XXVII.—*Notice of the discovery of Desmarestia Dresnaji on the coast of Ireland.* By R. K. GREVILLE, LL.D. &c.†

[With a Plate.]

THE Alga to which this notice refers was collected towards the close of last year, at Moville, near the mouth of Lough Fyle in the north of Ireland, by William Sawers and — Morrison, Esqrs., and communicated by the former gentleman to Professor Balfour, by whom specimens were placed in my hands for examination. It is a form quite new to the British flora; and as its affinity is involved in some obscurity, its discovery on our shores is invested with considerable interest. Specimens transmitted to the celebrated French cryptogamist, Dr. Montagne, have been pronounced by him to be identical with an Alga found by himself at Fort St. Sebastian in 1823, and published in the 'Annales des Sciences Naturelles' for 1842, p. 251. t. 7. f. 2, under the name of *Desmarestia pinnatinervia*. Dr. Montagne obtained only a single individual, scarcely more than 4 inches high, fully 2 inches wide, and truncate; being evidently an abnormal development. M. Cronan has likewise met with it, though rarely, at Brest, and regards it as a variety of *Desmarestia Dresnaji* of Lamouroux,

\* Memoir read before the Geological Society of London, Nov. 3, 1852.

† Read before the Botanical Society of Edinburgh, January 12, 1854.